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** "An Essay on Muscular Action and Its Conditions.. by J. H. Watters, M.D., Professor of Physiology in the St. Louis Medical College." (caption to opening chapter.)

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AN ESSAY

ON

ORGANIC, OR LIFE FORCE.

WRITTEN FOR THE DEGREE OF DOCTOR OF MEDICINE IN THE UNIVERSITY OF PENNSYLVANIA.

BY

J. H. WATTERS, A. B.

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AN ESSAY

ON

MUSCULAR ACTION AND ITS CONDITIONS.

THE NINTH OF A SERIES OF ARTICLES PUBLISHED IN THE ST. LOUIS MEDICAL JOURNAL,

ON

"LIFE,"

BY

J. H. WATTERS, M.D.,

PROFESSOR OF PHYSIOLOGY IN THE ST. LOUIS MEDICAL COLLEGE.

No. IX.—*Muscular Action and its Conditions.*—We have already considered the fact that Life and Decay are proportional, and have applied the theory that the speciality of action in a living organism is due to speciality of adjustment, and that the vital actions are *reciprocal* with oxydation, to the phenomena of reproduction, nutrition, secretion, and of "dormant vitality." And I think it has been shown that these phenomena go far to establish the theory. We have not neglected, moreover, to direct attention to the established fact that as the conditions of oxydation are increased or diminished within certain limits, vital action is increased or diminished accordingly, and to show the bearing of all this class of facts upon the question at issue. I wish now to direct attention to the *facts* connected with muscular action, in reference to the same point. To this effect, I shall attempt to establish, 1st. That muscular action is necessitated, and is definite and determined according to fixed laws; 2d. That the cause of the *speciality* of action is adjustment; 3d. That the cause of

action is neither the "nervous influence" nor the inherent property of the muscular tissue, but that muscular action is reciprocal with the oxydation of the muscle itself; 4th. That the nervous system is an arrangement of nature by which this oxydation is placed under the control of certain conditions; and hence, by which muscular contraction is placed under the control of these conditions. This article will be devoted to the facts bearing upon these points. True, I do not expect to demonstrate these propositions, but I expect to show so strong a probability in their favor, that when this probability is taken in conjunction with the preceding induction—with the *a priori* presumption—it will amount to a demonstration of the original proposition.

First, then—*Muscular action is necessitated, and is definite and determined according to fixed laws.* It is scarcely necessary to insist upon this proposition, as few if any will question it. It is sufficiently established by the ordinary rhythmical movements of the heart and the peristaltic action of the intestines, but it is no less established by experiments upon the voluntary muscles; for, these muscles may be made to act by mechanical and chemical agency, and by electricity, at pleasure. The truth of this proposition renders muscular action a subject of study, in ascertaining its laws and physical conditions.

Second—*The cause of speciality of action is adjustment.* The truth of this proposition, I contend, follows directly from the first; for, if the action is necessitated, the condition of its acting necessarily in *this* way rather than in some other way, must be adjustment. But some one may object that the speciality may be due to a special property of the muscular tissue. This is clearly a *petitio principii.* Upon what does this *special property* depend, or what meaning do you attach to "special property" after having admitted necessity, if you do not mean a special physical condition in distinction from some other physical condition? If it were not for the truth of the two foregoing propositions, the conditions and laws of muscular action would not be a subject of study.

Third—*The cause of muscular action is neither the "nervous influence," nor the inherent property of the muscular tissue; but*

muscular action is reciprocal with the oxydation of the muscular tissue itself. Here the distinction is clearly drawn between the condition of the *speciality* of action, and the condition of action. This distinction is the central idea of all that is peculiar in this series of articles upon the subject of "Life." The want of this distinction is at the bottom of the correlation hypothesis; when a special action is observed, they infer a special *force* rather than a special *adjustment.* It is true, we are accustomed to assume absolute force as a starting point wherever we are unable or unwilling to trace antecedents farther, and hence so many forces or modifications of force, rather than modifications of adjustment. But I have attempted to show that we are unwarranted in this, both by reason and fact; and that antecedent action or dis-equilibrium rather than *force* is our starting point in studying phenomena; and that reciprocal action is the law, and special adjustments the condition, of modifications of action. We will now consider separately the points of this third proposition.

(a) *The cause of muscular action is not nervous influence.* It is a *fact* that in the animal organism, the nervous system affords a condition of muscular action. From this fact, it has been inferred that the nervous system affords the *cause.* This inference is an hypothesis, because it is not supported by premises. A *condition* is not necessarily a *cause.* Air is a *condition*, and a necessary condition, of a bird's flying, but it is not the cause. Facts obtained by experiment establish that the nervous system furnishes only a condition, and not the cause of muscular action. Among these is the fact that the regular rhythmical movements of the heart may continue after all connection with the spinal centres is destroyed. But it may be, and has been, said, that centres of the sympathetic nerve, located in the substance of the heart itself, supply the cause. But this is merely guess-work for which there is no foundation. For, if the *voluntary* muscles be separated from their nervous connection with the spinal centres, they will contract under the influence of the point of a scalpel applied to the nerve trunk. Now, the point of the scalpel is a condition but not the cause of muscular contraction; and as the scalpel substitutes the spinal centres, the spinal centres are a condi-

tion but not a cause. Hence, admitting the centres of the sympathetic in the substance of the heart, these centres may be a condition, but there is no reason to believe they are the cause of the heart's action. Moreover, separate muscular fibres will contract under certain physical conditions. All these facts go to establish that the *cause* of muscular action is not nervous influence.

(b) *The cause of muscular action is not an inherent property of the muscular tissue.* "Thousand to one, the goal of your philosophy will be the spot where you become weary of thinking any further." The truth of this remark is nowhere better illustrated than in the result upon the philosophy of muscular action, following the establishment of the fact that this action is not *caused* by nervous influence. This goal of philosophy is always indicated by the assumption of absolute force as the proximate cause of phenomena. It is no more indicated by the phrase "nature abhors a vacuum" than by the terms "attraction," "affinity," &c.; and no more by these than by the terms "irritability," "excitability," "contractility," &c. No sooner is the independence of muscular action of the nervous centres established, than the phenomenon is explained by the assumption that contractility is a property which the muscle possesses in and by itself. This "contractility" is an assumed cause or *absolute* power, inherent in the muscular fibre. This is the manner Dr. Carpenter explains muscular action, and I will make a few quotations to show clearly his position. Having considered the objections to the doctrine, that muscular action is caused by nervous influence, he continues: "When all these considerations are allowed their due weight, we can scarcely do otherwise than acquiesce fully in the doctrine of Haller, which involves no hypothesis, and which is perfectly conformable to the analogy of other departments of physiology. He regarded every part of the body which is endowed with irritability, as possessing that property in and by itself; but considered that the *property* is subjected to *excitement* and *control* from the nervous system, the agency of which is one of the *stimuli* that can *call it into operation*."* Call a property into operation! Does this "in-

* Human Phys. § 327. Last Am. Ed.

volve no hypothesis?" It may be "perfectly conformable" to Dr. Carpenter's philosophy, and it is "perfectly conformable to the analogy of other departments of physiology" and physics, to assume absolute force as the proximate cause, at the spot where the mind becomes weary of thinking any further. But does this involve no hypothesis? The Doctor *will* take "what is held" as his starting point. Again, having considered the various doctrines which refer the movements of the heart to the nervous system, he says: "A more satisfactory mode of accounting for the rhythmical movements of the heart, appears to the author to lie in regarding them as an expression of the peculiar *vital endowments* of its muscular tissue; and to believe that, so long as this tissue retains its integrity, and the other necessary conditions are supplied, so long is an alternation of contraction and relaxation the characteristic and constant manifestation of its vital activity—just as ciliary movement is in cells of one class, and secreting action in those of another. The fact that this movement is seen to commence in the embryo heart, when as yet its parieties consist of ordinary cells, and no nervous structure exists either in its own substance or in the body at large, is an important confirmation of this doctrine; whilst the same fact stands in complete opposition to the idea, that the nervous force is in any way concerned in maintaining this rhythmical action. But, it may be said, that in attributing to the muscular structure of the heart a self-moving power, we really only throw back the question into the obscurity from which the physiologist has sought to draw it. Such is *far* from being the case, however, if it can be shown that this self-moving power is nothing else than an exertion of ordinary muscular contractility under peculiar conditions, and if analogous phenomena can be shown to present themselves elsewhere."* It is perfectly clear from these quotations, that Dr. Carpenter considers the doctrine, that muscular action is due to contractility which the muscle possesses as a property in and by itself, the only alternative after it is established that it is not due to nervous

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*Human Phys. § 498. Last Am. Ed.

influence. We will now analyze this doctrine and compare it with facts, that its merits, if it have any, may be seen ; remembering that the only evidence adduced in its favor is the evidence against the doctrine that muscular action is due to nervous influence ; this doctrine being disproved by facts, the doctrine we are now considering has been assumed as the *necessary alternative*. It rests, therefore, entirely upon this negative evidence.

" When all these considerations are allowed their due weight. we can scarcely do otherwise than acquiesce fully in the doctrine of Haller. * * * * He regarded every part of the body which is endowed with irritability, as possessing that property in and by itself." To get a clear idea of this doctrine, we must analyze the terms " irritability" and " property," and see what they really mean.

It is a fact of observation, that parts of the body, under certain conditions, perform certain actions. The susceptibility of these actions under proper conditions, has received the general expression, "*irritability*." A *property*, in science, means nothing more nor less than a quality or attribute inseparable from its subject, which cannot be increased or diminished, or in any way changed, without a corresponding change of the subject. Hence, it involves no hypothesis to say, that every part of the body which is endowed with a *susceptibility* of *action*, possesses this as a *property* in and by itself. This is merely a general expression of the fact. But when " irritability" is advanced as the *reason why* a muscle contracts, the spot is at once recognized, " where you become weary of thinking any further." You have arrived at the goal of your philosophy. Here is hypothesis, and nothing but hypothesis. The term irritability is sophistically endowed with a transferred meaning from that of *susceptibility* of action, which involves no hypothesis, to that of *cause* of action, which is all hypothesis. Now, Dr. Carpenter uses the terms *irritability*, *contractility*, *motility*, &c., in this sophistical, transferred sense, and it is to this I object. I expect to show that the *cause* of muscular action is *not* a property which the muscles possess in and by themselves. By the term "*cause*," is not meant the *ultimate* cause, nor the yet *final* cause, with which

the physical sciences have nothing to do, but the *proximate* cause or immediate antecedent with which the action is reciprocal as the *effect*.

Now, with clear ideas of the meaning of the terms "property," "cause" and "irritability;" having determined that the *cause* of muscular action is not derived from the nervous system, let us consider the doctrine that the *cause* is a *property* which the muscle possesses in and by itself, with the facts in the case. It is a fact of observation, that in the animal economy muscular contraction is followed by relaxation; this whole rhythm must be taken into the account. Your theory of muscular action must embrace not only the contraction, but the relaxation also. If the contraction be due to the property of "motility," what becomes of this *property* in relaxation? Dr. Carpenter, continues,—"the property is subjected to *excitement* and *control* from the nervous system, the agency of which is one of the *stimuli* that can *call it into operation*." This is the manner *he* accounts for the fact that the *property* (according to his hypothesis) is not all the time in operation. In regard to the rhythmical movements of the heart, he says: "Now just as the Leyden jar may be so charged with electricity as to *discharge* itself spontaneously, so it is easy to conceive that the muscle may be so *charged* with motility (or motor force) as to excite spontaneous contractions."* Again, "It is not *very difficult*, then, to conceive, that the ordinary rhythmical movements of the heart may be due to a simple excess of this motility, which is continually being *supplied* by the nutritive operations, and as continually *discharging* itself in contractile action."† Let us put *this* and *that* together. The *force* or proximate cause of muscular contraction is a property which the muscle possesses in and by itself; this *force* and this *property* is subjected to *excitement* and *control* from the nervous system; the muscle is *supplied* or *charged* with this *motility*, this *force*, this *property*, as the Leyden jar is charged with electricity; and as the Leyden jar is discharged, the muscle is discharged of this motility—this property, in contractile action. I claim that all this is contrary to first

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* Human Phys. § 499. † Ibid.

principles. Neither a *force* nor a *property* is subject to *excitement;* nor can any substance be charged with or discharged of a property. A property has no existence independently of the concrete subject, nor can a property be added to or separated from its subject. I consider these propositions self-evident, and therefore need not dwell upon this point. If they are true, Dr. Carpenter's doctrine is simply absurd. Would he contend, that the electricity with which the Leyden jar is charged, is a property which the Leyden jar possesses in and by itself? If "motility" can be "excited" or "discharged," it cannot be a property which the muscle possesses in and by itself; and conversely, if it be a property, it cannot be excited or discharged. A few words now in reference to this *method* of explanation.

Dr. Carpenter says: "But it may be said, that in attributing to the muscular structure of the heart a self-moving power, we really only throw back the question into the obscurity from which the Physiologist has sought to draw it. Such is far from being the case, however, if it can be shown that this self-moving power is nothing else than an *exertion* of ordinary contractility under peculiar conditions, and if analogous phenomena can be shown to present themselves elsewhere." I am at a loss to see how this objection is removed by showing that the self-moving power claimed for the heart is nothing else than the exertion of ordinary contractility. This is the method adopted;—true, to say the motions of the heart are due to its self-moving power, is no explanation; and true, to say the same of any *one* muscle in the body, is no explanation, but throws back the question into the original obscurity; but here I will generalize, and give a general expression to this self-moving power, under the name of "contractility;" if I now refer the motions of the heart to this general expression, '*contractility*,' I am *far from throwing the question back into its original obscurity*. Sublime philosophy! beautiful method of explanation! It is quite a refinement of the old method of explanation by 'occult properties!' It is no explanation to say that opium produces sleep on account of an occult quality which disposes it to produce sleep; nor is it any explanation of muscular action to say it is due to occult pro-

perty, '*contractility*,' which disposes the muscle to contract. Now, if muscular action in general is not explained by this occult quality, it is an accommodating philosophy which would explain the actions of a particular muscle by it. To explain any phenomenon of nature, we must establish a proximate cause *more general* than the *class* of phenomena to be explained. For instance, we explain the motion of a watch when we refer it to the recoiling of the spring, because the recoiling of the spring is more general than this *class* of actions; that is, the motion of the watch does not exist independently of the spring, but the recoiling of the spring may exist independently of the watch; but it is no explanation of the recoiling of the spring to refer it to 'elasticity,' because elasticity is not more general than elastic bodies; that is, 'elasticity' is but a general expression of the fact, and can only be equally general with that class of facts. The pressure of the atmosphere is an explanation of the rise of a liquid in a tube from which the air is exhausted, but 'gravitation' is no explanation of the atmospheric pressure, more than 'chemical affinity' is an explanation of chemical action, or than 'irritability,' 'contractility,' or 'motility,' is an explanation of muscular action. These are not proximate causes, but further than they express the generalized facts, they are but flights of the imagination indicating the 'spot when we become weary of thinking any further.' When the anchor of reason is weighed at this spot and free sail given to the imagination, it seems allowable to assume any thing and every thing; hence, Dr. Carpenter talks about 'irritability which the muscle possesses as a *property* in and by itself,' being 'stimulated into *active operation*,' and about the '*property* of motility' '*discharging* itself in contractile action.' Now, if muscular action is not due to nervous influence, nor to an occult quality of 'contractility,' what is the proximate cause?

(c) *Muscular action is reciprocal with oxydation of the muscular tissue itself.* If it is required of me to establish this proposition by *direct* argument, more is required than I shall undertake, and than entered into my original plan. A process of inductive reasoning resulted in the general proposition, that *vital action is reciprocal with oxydation.* If an explanation of

the phenomena of muscular action can be deduced from this, the general proposition is confirmed. Hence, my object now is not directly to establish the particular proposition that muscular action is reciprocal with oxydation, but I am now occupied with the *indirect* part of the argument to establish the general proposition, that *vital action is reciprocal with oxydation*. I have shown that existing theories do not explain the phenomena of muscular action. I expect now to show that these phenomena may be explained by the theory that muscular action is reciprocal with oxydation, and that the facts connected with the subject are consistent with this view. We are now concerned with the *indirect* part of the general argument to establish the general proposition; and if the general proposition is established, the particular is included. Hence, the point at issue in the present article is not whether muscular action is reciprocal with oxydation; but whether, upon the supposition that muscular action is reciprocal with oxydation, can an explanation of the facts be deduced? If the facts will justify us in referring this action to chemical changes in the muscular tissue, this will be an explanation, because chemical change is antecedent to mechanical power independently of the particular arrangement in the muscular and nervous systems. For instance, the power of a steam engine and of a galvanic battery is referred to chemical action as its antecedent. And I imagine it about as philosophical to say that as a steam engine *discharges* itself of its *motility*, which it possesses as a property in and by itself, in mechanical action, the wood under the boiler, no longer protected from chemical forces,—its protection being metamorphosed into gross palpable motion,—proportionally oxydizes; as to say, that as the property of motility which the muscle possesses in and by itself, *discharges* itself in contractile action or is metamorphosed into motion, the tissue being no longer protected from the influence of chemical forces, disintegration in proportion is a *consequence*. I am inclined to doubt the propriety of laying aside, as of no use, ideas derived from physical phenomena in general when we come to consider the phenomena of living beings;—

> "Well nigh the shore, where one stoops down and gathers
> Some pretty shell, is best for moderate bathers."

To consider the "nervous system as one of the *stimuli* that can call into *operation*" the irritability or motility of a muscle, seems to me to involve quite as much hypothesis as to say that the poles of a battery or the oxydizing fluid are stimuli that can call the motility of machinery into operation,—*motility* which the machinery possesses as a property in and by itself. In the works of art, when useful objects are accomplished by means of machinery not guided by intelligence directly, the distinction is always to be drawn between the *cause* of the specialty of action or the machinery, and the cause of action or the motor. Does it seem to you altogether self-evident that this distinction does not extend just a little farther than *man* is capable of adapting means to ends? May not the distinction be carried out even in the works of nature where the adaptation of means to ends had its origin in the Divine conception? Referring to the previous articles for the direct arguments which have led me to believe that these principles obtain in living beings,—that all vital action has oxydation for its antecedent proximate cause of action, and adjustment or special arrangement of physical agents for the cause of the specialty of action, I now pass to the consideration of the next general proposition of the present article, to see how far the facts are consistent with this view.

Fourth—The nervous system is an arrangement of nature by which oxydation in the muscular tissue is placed under the control of certain conditions, and hence, by which muscular action is placed under the control of these conditions.

We have seen that the antecedent to muscular power is connected with the muscles themselves, and that when this antecedent proximate cause exists, *definite* action is necessitated in consequence of the special physical arrangement. Also, that 'irritability' or 'motility' is not a *power*, but a passive quality; and that all theories based upon the idea of exciting or stimulating this passive quality into 'active operation' through the nervous system, or mechanical, or other agency, is based upon an absurdity. Now, if the proximate cause of muscular contraction is the chemical change between the fluids and the muscular tissue itself, how is this chemical action regulated so that contraction may occur only as occa-

sion calls for it? I only propose answering this question in a general manner, as our knowledge at present of the nervous system will not justify any thing more. As I am now considering the phenomena of muscular action in reference to the views advocated in the previous articles, the explanations here offered will be appreciated more according to the stand-point from which they are viewed, than to the direct arguments by which they are attempted to be sustained. If from the stand-point which you occupy, you see nothing absurd in *all* the explanations where the object is to be accounted for the *excitement* or *stimulation* of the muscular irritability, the explanations here attempted, where the object is rather to account for the regulation of chemical change in the muscle, will have but little force.

As soon as the phenomena of electricity began to receive attention, the identity of "nervous influence" and electricity began to be believed. I will pass over all those relations and corresponding phenomena upon which this belief was founded, as they can be read elsewhere. Neither will I refer here to those experiments which seemed to confirm, but only to those which have been thought sufficient to *disprove* this identity. In the consideration of these, I hope to present clearly what I consider the respective relations of the muscular and nervous systems to muscular action.

All phenomena are seen according to the relation of ideas which pre-occupy the mind. Let it be remembered here, that all the observations and experiments bearing upon the subject now under consideration, have been made upon the pre-conceived notion, that when muscular action follows an act of volition, *some* "influence" is sent from the nervous centres to the muscle. As in the early history of Astronomy, many valuable observations were made with the pre-conceived notion that the earth was fixed, and that the apparent motions of the heavenly bodies were real; so, many valuable facts have been gathered in regard to the function of the nervous system with this pre-conceived notion of the *transmission* of an "agency" or "influence" from the nervous centres. It is hard, and indeed impossible, suddenly to change the relation of our ideas; and hence, every conception which requires to

be viewed from a stand-point different from what we are accustomed, will be met with just such objections as met the Copernican theory,—"if the earth turn upside down, every thing on its surface would fall off." The present is emphatically an age of observation and experiment; but it seems to be lost sight of, that the results thus obtained are always shaped and colored according to the ideas pre-occupying the mind. Errors, now known, obtained so long because it was not suspected but that the color and shape derived from the mind were real and objective. All men theorize, and none more dangerously and erroneously than those who do not know it,—calling themselves "*Positive Philosophers.*" Does not observation prove that the earth is fixed, and that we are on the upper surface, while the sun, moon and stars revolve around us? Do not observation and experiment prove, that an impression made upon the fauces is *communicated* to the medulla oblongata and there *reflected* to the muscles of deglutition? Do not experiments prove that the "*afferent*" nerves conduct the "nervous influence" to the nervous centres, and that by these centres it is reflected to other parts through the "*efferent*" nerves? Does not the paralysis following the division of the motor nerves show that the centres cannot longer *transmit* their "influence" as in health? All this is the conception of those Philosophers who call themselves "*Positive*," and who imagine themselves free from hypothesis. But this view of the *modus operandi* of the nervous system is by no means established beyond question. Think of that! It is only taken for granted, without proof. It is hypothesis, perhaps fiction. It may be that muscular action is consequent upon oxydation of the muscle itself, and that this oxydation is only *regulated* through the nervous system. Until this view is at least shown improbable, the other should not be taken as absolutely true, as has been done for the foundation of experiments. If this view be true in nature, the question in regard to muscular "*irritability*" and the nervous influence, as the "cause" or as "one of the *stimuli*" to call it into operation, is entirely done away with; as also the question of identity of the "nervous influence" and electricity. If the nervous system, in relation to muscular action, is only an arrange-

ment through which oxydation of the muscle is controlled according to conditions, no more "influence" is sent down from the nervous centres to "stimulate" muscular irritability than is sent down from the poles of a battery to *stimulate* the oxydation of zinc; and nervous influence and electricity are no more identical than the *wire influence* of a galvanic battery is *identical* with electricity. But the oxydation in a galvanic battery may be controlled through the copper wires, and so muscular action is controlled through the nervous system. This view of the subject seems to remove many difficulties, and is consistent with principles and phenomena which are incompatible, as we have seen, with other theories. It seems to me, that the whole difficulty in regard to the *nature* of the nervous influence is due to this notion or fancy that somewhat, originating in the nervous centres and peripheral expansion of the sensory nerves, is transmitted *thence* by the nerve trunks to *excite* parts to which they are distributed.

The manner in which certain experiments, which we will now consider, are interpreted as proof that the "nervous influence" and electricity are not identical, illustrates the power of that assumption over the mind. In fact, all the experiments upon the nervous system have been made from that point of view. Now these experiments prove this, that the scientific world assumes that some agent or "influence" is sent down from the nervous centres to the muscles, in muscular action. I wish to show that this is a false assumption, and that there is reason to consider the relation between the nerve trunks and muscular action, the same as the relation between the wires of a battery and galvanic action. All the phenomena seem to indicate this view of the subject, while those now about to be considered may be taken as *instantiæ crucis*. If oxydation of the muscle be the antecedent upon which the motor power of the muscle depends, it is apparent that *all* conditions which promote muscular action must be conditions to promote oxydation.

First—"Prof. Matteaucci, having experimented upon the very large crural nerve of a horse, which was caused by *stimulating* (!) its roots, to throw the muscles of the leg into violent contraction, nevertheless found that, although he used

instruments of such delicacy as to be capable of detecting an infinitesimally small disturbance of the electric equilibrium, no such disturbance was evident." From such experiments as these, it is said that "all attempts to prove the existence of an electric current in a nervous trunk that is actively engaged in *conveying motor influence* (!) have completely failed though made with the greatest precaution;"* and hence the conclusion. Now these experiments of Prof. Matteaucci are made upon an assumption inconsistent with the laws of electricity. The fact is well known that a current of electricity passing along a conductor, may easily be detected; but the fact is equally well known, that if the conductor be bent upon itself so that the current passes in opposite directions at the same time, the equilibrium is restored, so that there is no means in our power of detecting such a current, how powerful soever it may be. According to the laws of galvanic electricity there must be a continuous circuit, and if "by *stimulating* the roots" of the crural nerve, an electric current is "*sent down*" to the muscles, there must be an equal current *sent back* to the point of "*stimulation*." But we have no reason to believe that electricity is developed at the point of "stimulation;" we rather have reason to believe that the means employed were a condition to the oxydation of the muscles, and that by means of the fibres and stimulus used there was a continuous circuit of electricity in the crural nerve trunk. If the agency of the nervous system in regard to muscular action is merely to place the oxydation of the muscles, and consequent evolution of electricity, under certain conditions, it would be expected that the experiment of Matteaucci would fail to detect the current, how powerful soever it might be.

Second—"If a small piece of a nervous trunk be cut out, and be replaced by an electric conductor, electricity will still pass along the nerve; but no nervous force, *excited by stimulus above the section*, will be propagated through the conductor to parts below." Here, again, is the absurd idea of *exciting* nerve force by a stimulus. The objections to the conclusion

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* Carpenter.

from this experiment are the same as to the first. The whole weight of this rests upon the absurd hypothesis, that nervous force is *excited* by a *stimulus* above the section, and propagated through the nerves to parts below. If the conductor placed in the section united the proper conducting portion of the nerve fibres, oxydation and contraction of the muscle would result in consequence of this alone, and it would itself be considered a "stimulus;" if it does not so unite the nerves, no mechanical impression above could induce the effect. But this conductor placed in the section so connects with the cellular tissue, surrounding fluids, &c., as to conduct electricity externally applied.

As all the objections against the identity of "nerve force" and electricity contain the same fallacies, we need not dwell upon them more, since it will be apparent already that these objections do not apply to the theory, that muscular motor force is electricity, and is evolved in the oxydation of the muscle. Upon this theory, it would be expected, too, that "mechanical and chemical *stimulation*" would occasion muscular action. Mechanical impressions will induce the contraction of muscles; so the circuit of a galvanic battery is completed by mechanical impressions. If the conductors, for instance, terminate within a short distance of each other, the circuit might be completed by the point of a scalpel; and thus, to us, the phraseology of physiologists, the peculiar *wire influence* or agency excited by the point of a scalpel, is transmitted to the "parts below," to produce or excite galvanic battery action; and, as the power which resisted chemical action in the battery is thus metamorphosed into gross motion, oxydation of the zinc results as a necessary *consequence!* The nervous agency and electricity are, therefore, not identical; but the *agency* of the nervous system is to place the oxydation of the muscles, and consequent muscular action, under the influence and control of certain internal and external conditions. We will now consider these conditions as illustrated in the phenomena of muscular action. Muscular action, depending upon oxydation, may be regulated by two classes of conditions, viz: those connected with the nervous system, and those connected with the oxydizing fluid. One or the other, or both of these

classes of condition, is adopted in any particular case according to the kind of action required. In reflex and voluntary action, the oxydation is regulated through the nervous system; in the rhythmical action of the heart, it is regulated by the oxydizing fluid in a great measure.

First—*Of "Reflex Action."*—Dr. Carpenter expresses the received views of the *modus operandi* of "reflex action" in the following words: "By contact, pressure, or some other form of mechanical agency, an *impression* is made upon the peripheral extremities of the *afferent* nerves; and this impression, or the change induced by it in the condition of the nerve fibre, is *transmitted* by the nerve trunk to the central ganglion. In this ganglion, the influence transmitted by the afferent trunk *excites a re-active change;* the occurrence of which is indicated by the *transmission*, along the *efferent* nerves, of an *influence*, which, being distributed to the muscular substance, *excites* it to contraction."

The above quotation shows most beautifully the present mode of substituting words for ideas, in explaining the function of the nervous system in relation to muscular action. Now, further than the fact that through the "afferent" and "efferent" nerves, and the ganglion, the "contact, pressure, or some other mechanical agency" is a *condition* to muscular contraction, this is but the flight of a highly wrought imagination. How is it known, that the "impression, or rather the change induced by it in the condition of the nerve fibre, is transmitted by the nerve trunk to the central ganglion;" and how is it known, that "in this ganglion the influence transmitted by the afferent trunk excites a re-active change;" and how is it known, that an influence is transmitted "along the efferent nerves, which, being distributed to the muscular substance, *excites* it to *contraction?*" If the *fact* that "contact, pressure, or some other form of mechanical agency upon the peripheral extremities of the afferent nerves," is followed by muscular contraction, *proves* all this, then the fact that pressure, contact, or some other form of mechanical agency upon the extremities of the conductors of a galvanic battery, is followed by chemical action in the battery and electric phenomena, *proves* that this "impression," or rather the change

induced by it in the condition of the "conductors," is *transmitted* by the "conductors" to the battery, and there *excites* chemical action and electric phenomena; or rather, according to the hypothesis we have been considering, excites electric phenomena, and chemical action results in consequence. I cannot but believe that the facts are much more naturally and satisfactorily accounted for by the view here taken of the respective functions of the muscular and nervous tissues, upon principles established and recognized apart from these phenomena of the animal economy. The "contact, pressure, or some other form of mechanical agency," causes a physical change in the extremities of the "afferent" nerves, which physical change is a condition through the nerve conductors of a corresponding change in the ganglion, just as by "contact, pressure, or some other form of mechanical agency," a physical change is produced in the extremities of the wire conductors of a galvanic battery, and is thus a condition of action in the battery. As the action in the galvanic battery may by special adjustment as in the electric telegraph, unite the *poles* of a second battery, so the action in the central ganglion causes a physical change in the extremities of the "efferent" nerves, which change is a *condition* to the oxydation and consequent contraction of muscles. As intimated before, I only aim at general principles here; a more exact knowledge of the minute arrangement of the nervous system and of the conditions of electric phenomena is required before we can specify with exactness the peculiar physical change in the ganglia and extremities of nerves. These general principles follow directly from the previous arguments. When an impression is made upon the extremities of the "afferent" nerves, muscular action is *necessitated* through the nervous connection; this *necessity* proves adjustment; but adjustment is not power, yet through adjustment action is modified. The adjustment in the automatic portion of the nervous system, is for the regulation of the oxydation of the muscle according to circumstances. In deglutition, the substance to be swallowed, pressing upon the nervous expansion in the fauces, causes such a change, that through the "afferent" nerve conductors, it is a condition of a change in the medulla oblongata, without

any influence, agent, or impression being sent up; this change in the medulla, through the efferent nerves is a *condition* to the oxydation of the muscles and the consequent act of deglutition. According to this view, we have, both in the "afferent" and "efferent" nerve trunks, counter currents; consequently, such experiments as made by Prof. Matteaucci upon the crural nerve of the horse, would necessarily fail to detect the presence of electricity. And the quantity of electricity conducted even by the "efferent" nerves may be extremely small; for, as the manner in which these terminate is not yet definitely known, they may be merely instrumental in producing a change in a third set of conductors terminating in more immediate connection with the muscles themselves.

Second—*Of Voluntary Action.*—The points at issue in this article have nothing to do with the relation of Mind and Matter, nor yet with the *exact* physical change in the nervous centres antecedent to muscular action. We only aim at general principles as a point from which to view the phenomena. The mind wills *results*, and not the action of special muscles. The mind may not know that muscles exist, much less that nerves pass from the muscles to the nervous centres, and much less still, the origin of a special nerve fibre in the nerve centre; and yet the action is determined as definitely as if all this were known. Hence, the special action following upon the Will is necessitated, and consequently, the relation between the Will and muscular action depends upon adjustment. This view is conclusive, whatever may be your notion of nervous agency. If the Will "*sends down*" an "influence," the "influence" is determined by adjustment to the axis-cylinder of one fibre rather than another. The mind has nothing to do with the connection between the will and the result. Now, if the motor nerves be divided and a *mechanical impression* be made upon the extremity towards the muscles, contraction results; hence, in connection with the will some change takes place in the nervous centres which may be substantiated by this mechanical impression. Hence the arguments and views advanced in regard to "reflex" action, apply here. Adjustment is not power. The changes in the nervous centres in connection with the Will, as with the mechanical impression, only

determines the oxydation of the muscle and the development of power. The action of the will, as of every mental action, is accompanied with molicular change; hence, we can understand how the will may be a condition to muscular action, as well as how the substance in the fauces, or the point of a scalpel, is a condition to muscular action.

Third—*Muscular Action regulated more especially by the oxydizing fluid.*—If muscular action depends upon oxydation, whatever would modify oxydation would modify the consequent action. While the classes of actions just considered are regulated through the nervous system, conductors, or poles, that class now about to be examined seems to be regulated by modifications in the blood. I will take the rhythmical movements of the heart as a type of this class. Not that I would contend that "reflex action" has nothing to do with the actions of the heart, more than I would that the oxydizing fluid has nothing to do with "reflex" and "voluntary" action, but only that the rhythmical movements are otherwise regulated. The heart, in contracting, forces the blood not only from its cavities, but from its substance also—each fibre *discharges* itself by its own action. As the heart thus discharges itself, the oxydation and consequent contraction cease, and dilatation follows from the power which had been overcome in the contraction. Upon the dilatation, not only are the cavities filled again, but each fibre is again *charged* with blood, and thus the rhythmical movements continue in beautiful order! How simple, and yet how beautiful is this arrangement. Even the fibres of the voluntary muscles discharge themselves to some extent in contracting, but other portions contract alternately to compensate. The object of the contractions of the heart is evidently to force the blood from its cavities; but the very action which accomplishes this object, removes the condition of the evolution of the contractile force; and while the cavities are again being filled, this condition is again restored by the contraction of the aorta forcing blood into the substance of the heart. Thus the actions of the heart are regulated by the conditions of the blood, and accord with the objects to be accomplished. I would direct attention here to the position of the orifices of the coronary arteries. They are

so situated in the aortic sinuses that blood cannot be forced into them during the systole of the ventricles, but during the diastole by the contraction of the aorta. According to the experiments of Dr. J. Reid, the rhythmical movements continue *in vacuo;* but this is negatived by the experiments of Drs. Mitchell and Bache, of Philadelphia; and it seems clearly established, apart from the fact that the action of a muscle is in proportion to its oxydation, that the presence of oxygen, or of some agent capable of producing molicular change, is essential to muscular action. No one would contend that a perfectly *dry* muscular fibre would undergo rhythmical movements; when fibres separated from the body undergo such actions, they may be *charged* with the fluid which gives them moisture, of which it is discharged by the contraction, and again charged with during relaxation.

I conclude that the idea of *irritability*, *excitability*, or *motility*, as a power inherent in the muscular tissue, or as derived from the nervous system, is a fiction; that the idea that any physical power can be dormant or inactive, and can be stimulated or excited by any agency whatever, is an illusion; that the idea that an "impression, agency or influence," is transmitted from the nervous centres to the muscles to *excite* them, is a false point of view from which the phenomena have been observed. I conclude that oxydation of the muscular tissue is the *source* of muscular power, and that all conditions which regulate muscular action are conditions to regulate this oxydation; that when permanent contraction is caused by electricity, the oxydation of the muscle is *substituted* by the oxydation of zinc. I conclude, that upon the supposition that oxydation is the source of muscular power, the phenomena of muscular action may be explained as far as our present knowledge of the laws of physical agents and of the minute structure or arrangement of the muscular and nervous tissues will permit; that it is not philosophic to assume peculiar forces and agencies till our knowledge of these laws and of the special adjustments in the organism justifies such an assumption; that in the present state of our knowledge, such assumptions are even as presumptuous as unphilosophic,—presumptuous, because based upon an assumed knowledge which we do not

nearly possess. I conclude, that the burden of proof is thrown upon those who contend that the phenomena of muscular action, nervous "influence," and of life in general, are incompatible with ordinary physical principles.

The views just advanced in regard to the agency of the nervous system in "reflex" and "voluntary" action, is equally applicable to the nerves of special sense. The external condition, whatever it may be, causes a change in the peripheral expansion of the nerve, in consequence of which no impression or influence is sent up to the brain, but an action takes place in the brain corresponding to the external change. How perception is connected with this change of the brain, must remain unknown till we know more of the relation of mind and matter; but the *fact* seems established, that the mental actions and molicular change in the brain are directly proportional. This view of the function of the nerves of special sense, has the same foundation as that of the "afferent" and "efferent" nerves given above.

There seems to be a gradual approach among physiologists to the views contained in this article. It is now a fixed doctrine, that muscular disintegration is in proportion to muscular action, but it has not been considered that muscular power is *consequent* upon this disintegration or oxydation. However, I find in Prof. Draper's new work on Physiology, published this year, the following remark upon this subject: "So far from there being any thing mysterious or incomprehensible about it," (the cause of muscular action,) "as some writers insist, we probably shall not be very far from the truth, if we assert, that *muscular contraction is the necessary result of muscular disintegration;* and without here considering the various ways by which that muscular disintegration may be brought about, such is the doctrine that I now present." If this is true, the conditions of muscular action must be conditions of muscular disintegration; but Dr. Draper has not applied this to the agency of the nervous system and the blood in regulating muscular action. Dr. Brown-Sequard, in 1853, proposed the theory, that cabonic acid in the blood is the excitant of the muscular contractility of the heart, and he explains the rhythmical movements by the doctrine that the

contraction of the heart forces the excitant from the small blood-vessels and capillaries, and that during dilatation they become filled again. Now, if you discard this fancy doctrine of exciting muscular irritability by any stimulant whatever, and substitute what is proposed by Dr. Draper, "that muscular contraction is the necessary result of muscular disintegration," and extend the idea of the heart, by its own contraction forcing the blood from the blood-vessels, to the muscular fibre itself, you have the theory I propose respecting the rhythmical movements of the heart. It may be proper to state here that I published the views contained in this article in a pamphlet in 1851, so that I only mention the views of Drs. Draper and Brown-Sequard to show that the views that I published in 1851, and that are contained in this article, are being approached by physiologists. It may be proper to remark too, incidentally, that a temporary increase of the heart's action following an increase of carbonic acid in the blood, does not prove that the carbonic acid is a "*stimulant*" to the heart's "irritability," as supposed by Dr. Sequard, but rather confirms the views contained in this article. An accumulation of carbonic acid in the blood would proportionally retard the capillary circulation and cause a distension of the arteries with blood, and hence the heart would be charged with more blood during dilatation, and the increased action would be in consequence of this, and not the direct stimulating effect of carbonic acid.

In the next article, we will consider the phenomena of disease in reference to the same position, that oxydation or disintegration is the cause of action, and that adjustment is the cause of speciality of action in living organisms.

I
Watters
1851